

From: [Robert Neely](#)
Reply To: [Robert Neely@noaa.gov](#)
To: [Alyce Fritz](#); [Ron Gouquet](#); [Helen Hillman](#); [Jay Field](#); [Nancy Munn](#); [nick iadanza](#); [Katherine Pease](#); [Chip Humphrey/R10/USEPA/US@EPA](#); [Eric Blischke/R10/USEPA/US@EPA](#); [Joe Goulet/R10/USEPA/US@EPA](#); [Jean Lee](#); [Chris Thompson](#); [Jeremy Buck](#); [Jennifer Peterson](#); [Bob Dexter](#)
Subject: NOAA comments on draft Foodweb Model evaluation report for Portland Harbor RI/FS
Date: 02/27/2006 02:49 PM
Attachments: [NOAACCommentsFWMDraft022406.doc](#)
[Robert.Neely.vcf](#)

NOAA comments on the foodweb model report are attached FYI and, as appropriate, for your administrative records. Text embedded below:

-R

This letter provides *NOAA's comments on the document titled **/Food Web Modeling Report: Evaluating TrophicTrace and the Arnot and Gobas Models for Application to the Portland Harbor Superfund Site/*, draft of November 4, 2005 and prepared by Windward Environmental L.L.C. for the Lower Willamette Group._

_

__*_General Comments:_*

NOAA reviewers found the report to be well written (we did note that some additional editing should be considered) and would like to recognize the substantial effort and work involved in developing this draft document. We also agree with the assessment, as presented, that the Arnot and Gobas model is a more appropriate choice for application to this effort, especially considering its enhanced flexibility and transparency relative to TrophicTrace. Hence, our review is focused solely on the evaluation of the Arnot and Gobas model.

NOAA was somewhat disappointed that the report does not provide more extensive analyses of the reasons for the models' failure to accurately predict concentrations in biota. As described in the report, two compounds are selected to test the model. Criteria for selecting these two compounds include a reasonably robust supporting data set and variability for a key parameter (K_{ow}) that can be used to help determine if the model(s) can be accurately parameterized. The test runs for the sensitivity and uncertainty analyses provide a substantial amount of information regarding how the models are running, but the report fails to fully discuss the reasons for the results and how those might affect the use of the models. To provide additional clarity, it may be useful to consider, for example, whether one or a few fish species were consistently modeled well. In those instances where this proves to be the case, consideration should be given to uniqueness (e.g., small home range, particular diet, etc.). Similarly, for species where the model does not perform well, what features unique to them may have resulted in low performance (e.g., small data sets, or data sets that might not be expected to accurately represent the modeled area)? By way of a more specific example to illustrate our point, consider the modeling of Swan Island Lagoon as presented in the report. In this instance, perhaps more weight should be placed on the accuracy of predictions for those species with substantial localized data that would be expected to be most "linked" to the lagoon through diet and home range.

In this same vein, it appears that more effort is required to explain why PCBs can be modeled with some degree of agreement between predicted and measured, but DDE is consistently under-predicted, even using what appears to be a high K_{ow} . (The Log K_{ow} given in Arnot and Gobas was 5.70, which also results in substantial under-predictions for fish in their application.) The conversion of DDT to DDE may partially account for these inconsistencies. However, there may be additional explanations for instances where source concentrations are not representative (e.g., the actual "active" layer of sediment was not measured, as noted in the report). To better determine the source(s) of under-prediction, NOAA suggests it may be beneficial to review and compare results for "best" and "worst" predicted species.

The draft report alludes to the lack of inclusion of a link between the sediment and water concentrations as a weakness in the Arnot and Gobas model. (NOAA is concerned that the significance of this weakness may be understated.) At the present time, concentrations in biota are influenced by concentrations in two sources, sediment and water, that are independent from one another. The report states in Section 6.3.4 that future work will be required to estimate the relative influence of these sources. However, an examination of the coding, as well as the results of the sensitivity analyses should give a fairly clear idea of which species will be estimated by the model to be water- or sediment-source dependent, as well as how that dependence can be altered by changing the diet. However, it is a reasonable expectation that the sediments in the Willamette River are contributing to the water concentrations and that in some locations or times, the inverse may be true. This issue is important because the fundamental use of the model will be to estimate the changes in biota concentrations resulting from the clean-up of one or more sources.

NOAA reviewers attempted, in a very limited effort, to track coding and parameterization, and compare those to some of the results presented in the sensitivity analysis. In some instances we were able to follow through to results for some of the tests. While acknowledging our effort was limited, it may be useful to check/verify some of the runs.

For example, in Table 4-16:

- A decrease in biota lipids by 50% is indicated to produce no change in phytoplankton concentrations, but our simple calculation (which ignored growth dilution because it looked small), indicated that the lipids in the plankton accounted for about 50% of the accumulation in the base case, so we would have expected to see a decrease.
- Later in the table, a reduction in the water content is also indicated to result in no change in phytoplankton uptake, but the reduction should have increased the NLOC substantially, resulting in increased accumulation.
- A decrease in the K_{ow} of 50% is indicated to produce a 16% increase in phytoplankton concentrations, but our calculations indicate that the reduction in accumulation by the tissue far outweighs the increase in water concentrations, resulting in a large decrease in the phytoplankton concentrations.
- The reduction in dissolved oxygen results in a predicted decrease in the concentrations in most biota, but we could only find DO concentrations affecting the uptake via changes in the ventilation rates and in the filtering rates of filter feeders. In both cases, decreasing DO concentrations increase the ventilation or filtering rates, which should result in increased uptake.
- When the PCB water concentrations are reduced, it is not clear why amphipods, which have a diet of some phytoplankton, and not oligochaetes, which had a 100% sediment diet, were least responsive.

Our final comment relates to the issue raised in the report concerning the lack of an area-use factor in the Arnot and Gobas model. It may be possible to address this concern with a fairly simple addition to the coding if necessary. On the other hand, there are likely better ways to handle the issue through the careful use of the model and interpretation of the output, for example, by adjusting the spatial range to address particular fish species, running the model for small areas for species with limited home range, then using those data as set prey concentrations for species with larger home ranges, or comparing the output to some pseudo area-weighted biota concentrations. These are just some thoughts on how this issue might be addressed.